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ORIGINAL COMMUNICATIONS.

SOME REMARKS UPON THE RELATIVE STRENGTH OF CHLOROFORM AND ETHER, WITH HINTS UPON THEIR USE AS ANÆSTHETICS.

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Read before the Philadelphia County Medical Society, October 14, 1874.

MR. PRESIDENT and Members of the Association. Gentlemen: Desirous of fulfilling, so far as I may be able, the obligations assumed in becoming a member of this Society, I have cheerfully, though with many misgivings, complied with a request from your Secretary to present a subject for this evening's consideration. I have chosen *the administration of anæsthetics*, a subject that must continue to be of interest while pain and disease are the heritage of man. But I am frank to confess that I introduce such a subject before this Society with very many misgivings, as some of your members have been familiar with these agents since their introduction to surgery. There is, however, in this a compensating pleasure, that we will have the advantage of listening to these gentlemen after my brief remarks are ended.

My practical experience with anæsthetics began with my appointment to the surgical clinic in the Jefferson Medical College in 1867. Here, through the kindness of the Professor of Surgery, I was given in charge of the chloroform, and for more than two years assisted him in that capacity in his public and private labors. I look back upon the discipline of those years as among the most valuable of my life. Professor Gross is a firm believer in the superiority of chloroform over all other anæsthetics, is careful to instruct his assistant in its use, and then gives him to understand that his duty is *wholly with the chloroform,—that he has no other duty,—and this duty he expects him faithfully and conscientiously to fulfil.* Herein lies the "good luck" of Professor Gross. I know of no other way of accounting for it; for at a public clinic, where every age and condition of life presents itself, one has certainly every right to look for disasters, and yet in all my service with him I recall but a single instance of apparent danger. A female, whose surgical malady was complicated with phthisis, was about to be operated upon, and, as she was tardily influenced by the chloroform, an assistant dashed on the towel a quantity from a bottle that would hold two pounds. The result in this case gave some alarm, and taught me—

1. Never to be in a hurry to effect anæsthesia.
2. Never to use a large quantity when the patient is nearly influenced.
3. To use a small vial, from which a small quantity can be dropped, and that quickly.

From this time forth I began to influence my cases rapidly, and with very little chloroform; but when it occurred to me to make some observations upon the amount necessary to effect anæsthesia in ordinary cases, the result was the construction of the following apparatus:

FIG. 1.

a

It consists merely of two tin cones soldered apex to apex, with a tube projecting from the upper or receiving cone into the lower. Around the base of the larger cone a piece of linen is tied. When the instrument is to be used, a towel is properly folded and pinned around the larger cone, presenting, when complete, a cone of sufficient size to cover neatly the mouth and nose. This cannot be called an inhaler with any more propriety than can a towel or a napkin; but, while it resembles the latter in principle, it has in practice very many and important advantages.

1. The chloroform falls through the tube (Fig. 1) upon a single layer of linen (*a*), to both sides of which the air has ready access, and is accordingly instantly evaporated.
2. Every drop is conveyed to the patient.
3. A *few drops* at a time are all that are ever required, and all the patient can breathe with comfort.
4. The dropping may be more or less constant, as the instrument need not be removed from the face, and by means of a "dropper" the operator can gauge the amount to the necessities of the case.
5. The anæsthetic influence is gained gradually, imperceptibly, and rapidly, and with a minimum amount of chloroform. I seldom use more than a drachm and a half in adults.
6. As only a few drops need be added at a time, the danger must be far less than when an indefinite quantity is poured on from a bottle at once.
7. There is no exclusion of the air, but the air that is breathed is impregnated with a fresh supply of chloroform.
8. The time consumed is usually from three to ten minutes.
9. The influence once obtained may be easily maintained.

I have used this repeatedly for nearly two years, and have found it all that I could desire. It is exceedingly convenient as a frame-work about which to pin the towel, and as a receiver of the chloroform, and so simple in construction that any tinsmith could repair it should it be broken.

As to its rendering the use of chloroform safe, I can only say that *safety does not lie in an inhaler*, but in *him who uses it.* I think, however, it is far safer to produce anæsthesia by an equal and constant supply of chloroform, *i.e.*, drop by drop, than by pouring it on *at intervals*, and in quantities even so small as half-drachms at a time.

When I use it, I drop my chloroform from a graduated bottle containing three drachms; and the operation must be long and severe that will require a refilling of my little vial. The whole apparatus is calculated to render one most watchful.

The facts that he holds in his hand a vial with three drachms of chloroform, a drachm and a quarter of which, properly used, is to effect complete anæsthesia; that every drop of the chloroform goes to his patient, and that to effect his purpose the supply must be constant; that as he watches the dropping he watches also its effects upon his patient,—such thoughts must, I say, keep the administrator on the alert, and he who watches his patient with such care will not often be embarrassed by unpleasant symptoms. *Too much care cannot be exercised by the chloroformist*, since Simpson says of it, "I have known seven inspirations from thirty drops to place a strong man perfectly under its influence."

As soon as I had satisfied myself with chloroform, I turned my attention to ether. In children, and in some females, I could effect rapid anæsthesia with the apparatus; but in the majority of cases I found it unsuitable. With it I could not give ether fast enough. One strong man to whom I was administering it kept repeating, "Give it to me faster; pour on the whole bottle," though I was pouring it on at the time in a constant stream, and it was running down the edges of the towel. To satisfy myself about it, I frequently inhaled it, and experienced the same desire for *more ether*. This experience suggested to me the necessity of a larger evaporating surface, and, as a result, the following apparatus.

It consists of a wire frame-work, sufficiently large to cover the lower part of the face. The wires are

FIG. 2.

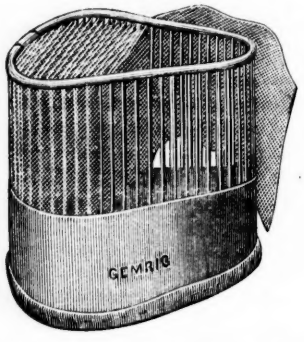

FIG. 3.

parallel, and about an eighth of an inch apart. Between the wires, from side to side, a strip of bandage two and one-half inches wide is passed. By reference to the figure (Fig. 2), the manner in which this is accomplished will be readily understood. Upon the right, a part of the wire frame-work is purposely left exposed to show the manner of its construction. The instrument is only about four inches long and three inches at its greatest width, and yet it consumes nearly three yards of bandage when passed between all the wires. By further reference to the figure, it will be seen that each section of the bandage is separate from the adjoining one, thus permitting the air to pass freely to both sides of it. When ready for use, the sides are covered, but the top is left open for the free entrance of air and for convenience in supplying the ether. I usually supply the ether from a graduated bottle furnished with a dropper-cork (Fig. 3), as by such a convenience I can add the ether as gradually or as rapidly as the case may demand.

My mode of using it is as follows. Placing it over the face, I sprinkle on *a few drops* of ether,—I mean literally but a few drops. In a few seconds I add a few more drops; and usually in from half a minute to a minute I find that I can drop it more constantly. As soon as I notice *the deep inspirations* I pour on a small stream, watching carefully lest I irritate the larynx, and as soon as I find the patient tolerant of its vapor I add it in larger quantities, and as rapidly as can be evaporated, and am usually gratified by seeing my patient pass *quietly* under its influence in from three to ten minutes.

Now, it may be asked, what advantages does this possess over the towel and sponge? First, let me speak of the disadvantages of the towel and sponge. The towel is usually fashioned into a cone, and placed in a similar cone of paper. The object of the paper cone is to prevent the evaporation of the ether, *so that the patient will get it all.* Now, it is quite certain that the patient does get all the ether, and that by means of the paper cone very little can escape. But it will be noticed that there is left but a very small evaporating surface, for the *inner surface* of the towel mainly yields the ether; and it is still further true that the more rapidly the ether is poured on—*i.e., the more fluid ether there is in the meshes of the towel—the less rapidly is the vapor of ether evolved.* The reason of this is obvious upon a moment's reflection. The moment the ether is poured on liberally, the towel becomes wet with it; its various folds, lying in close contact with one another, are filled with fluid ether, and it may not inaptly be regarded as a solid mass of ether, whose evaporation is only possible from the inner surface of the towel. This is a matter of easy demonstration. Take, for instance, two towels; fold one into a cone, and, investing it with a paper one, pour on an ounce of ether. Take another towel, and, suspending it so that its entire surface is exposed to the air, pour on a like quantity. The latter will be dry, and there will not be a trace of ether left in a minute's time; while the former will be damp and cold, and yield a strong odor of ether for at least five minutes.

With the sponge it is even worse. The sponge usually recommended is the finest that can be found. The pores, so to speak, are very small, and when two or three ounces of ether are thrown upon it it becomes a mass of liquid ether, with an exceedingly small evaporating surface, and hence wholly unfit for the rapid disengaging of the vapor of ether.

I return now to what I think I may in all fairness claim for the apparatus.

1. It possesses *a large evaporating surface.* You will remember that it consists of a bandage three inches wide and one hundred inches long, so arranged that in its entire length no fold touches its fellow, and with a free access of air to all parts of it. As compared with this, the evaporating surface of the towel or sponge is very small.

2. With a large evaporating surface there must be rapid evaporation of the ether, and consequently rapid anæsthesia. I have placed patients under its

influence in three minutes, seldom requiring ten minutes.

3. With rapidity of evaporation may be associated *economy* of ether. I usually require less than *two ounces* in adults, and rarely three ounces.

4. There is no exclusion of the air, and no unpleasant laryngeal irritation. I have rarely had a patient cough or spit during the administration, and it is extremely rare for patients to remark upon its unpleasant odor or object in the least to inhaling it.

In this respect it presents a marked contrast to the mode commonly adopted. It is usually considered important to exclude the air, and to give the patient "nothing but ether" to breathe. The result is that the patient resists as for life, and is only overcome by superior numbers.

Lastly, the inhaler need not be removed from the face; and, the supply of ether being constant and fresh, the patient passes under its influence quietly,—often without a struggle. Even after I have effected complete anæsthesia I continue adding it in small quantities,—a few drops at a time,—watching the degree of effect, which is usually indicated by the character of the respirations. A little experience will enable one to maintain throughout an entire operation a uniform degree of anæsthesia,—a condition very gratifying to the surgeon.

But an inhaler is not a convenient thing to carry with one in private practice; and, as a towel is usually to be had, allow me to make a suggestion in regard to folding and using it. (1) Spread out the towel, and over it place a newspaper of the same size; (2) make two folds of it lengthwise, the paper being folded within; (3) now double it again, so that it will be about ten inches long and eight wide; pin this into the shape of a cone. If, now, you examine this, you will find that there is but a *single layer* of the towel that can be wet with the ether, as the next layer is paper. Now, use this with the same care that I have enjoined in using the inhaler. Begin with a few drops, and add no more than *a few drops at a time* until the patient has become accustomed to the vapor, and then never add more than *a drachm at a time*, and be sure that this drachm is sprinkled over a broad surface. The supply of ether must now be quite constant; therefore, two or three times a minute *sprinkle* on a drachm, taking care not to *wet* the towel by adding a large quantity at a time, and you will be amply repaid for your pains in seeing the ease with which you effect your purpose.

As to sponges, a fine sponge is the worst thing that can be selected. The coarser the sponge, the better it will be found to be adapted for the purpose.

I will here insert the more important rules laid down by those most experienced in the use of anæsthetics. There should be no possible barrier to the freest respiration. Among the fatal cases from chloroform, one is recorded where "the neck-tie was removed as soon as the man became blue in the face."

The stomach, as a rule, should be nearly or entirely empty. If the stomach is distended either from food or drink, vomiting will almost invariably ensue, and vomiting when the patient is narcotized from any cause is not unattended with danger. Among the cases put down as deaths from ether will be found those that really are due to vomited matters passing into the trachea.

Often when the patient shows a disposition to vomit it may be averted by hastening the anæsthetic effect, and this is especially true of ether; but when vomiting is inevitable, the patient should be placed in the most favorable position for the escape of the contents of the stomach. It is not enough to turn the head to one side; the body should be turned on the side, and then the slight turn of the head downwards will be sufficient for all practical purposes.

It is strange how often this precept is violated. In one case, however, I even saw a doctor put his hand over the patient's mouth, to prevent the contents of the stomach from escaping upon the bedding.

*There is but one posture for the administration of chloroform, and this is the reclining. From this rule there is no safe departure.** Many of the fatal results from its use are from neglect of this cardinal maxim. Ether, on the contrary, knows no such law. When, however, the erect position is desired, let the anæsthetic be induced in this posture, for I question the safety of raising a patient thoroughly anæsthetized in the reclining posture to the erect. If the etherization is begun in the erect posture, its progress can be watched, and, in case of danger, the position can be changed.

The *degree of narcotism* will depend upon the object for which the anæsthetic is administered.

It is a well-known fact that a patient may cry and moan through an entire operation, and yet remember nothing of it at the return of consciousness. But the rule is, and it is founded upon the experience of surgeons, that severe operations should not be undertaken without first securing a profound impression from the anæsthetic.

Lastly. Leave the patient under the care of a competent, faithful attendant, with positive orders not to leave his presence until the effects of the anæsthetic have passed away. One of the most distressing cases that I have ever read was that of a man who had been operated upon for strangulated hernia, who, waking from the operation, snatched away the dressing and tore out in his frenzy yards of his own entrails.

Some begin with ether, and bridge over the exciting stage with chloroform. I would be afraid of this "bridge." Its farther side might be in eternity,—since it is nothing less than using a ten-fold more dangerous anæsthetic at the moment when danger is most imminent.

Out of one hundred and sixteen deaths from chloroform, three-fourths (88) occurred in this very stage of excitement.

Others use a mixture consisting of two parts of ether and one of chloroform. This is called chloric ether. This is used very generally by dentists, and

* As the design of this article is merely to offer a few hints upon the administration of anæsthetics, no theories are offered for the action or fatal results of the agents.

in the sitting posture. An ounce and a half of this mixture is usually sufficient to effect the desired purpose, *but its use cannot be too strongly condemned.* This ounce and a half, you must bear in mind, has half an ounce of chloroform, *and this is given in the sitting posture! and as freely as if it were only ether!*

"In the mixture of chloroform and ether, we have no definite chemical compound formed. When the mixture is exposed to air, the proportion of the ingredients is continually changing by the greater proportionate loss of the more volatile body, and it is quite impossible to apply any formula by which we can judge of the comparative rate of evaporation of the two ingredients."*

The union of the two ingredients may properly be called a mixture, and the mixture remains constant so long as it is excluded from the air; but the instant it is used for the purposes of anæsthesia, the more volatile ether first escapes, leaving behind it the more powerful but less volatile chloroform. I must not be understood as saying that the vapors do not mix; they certainly do pass off simultaneously. Nor can they be said to separate. But the important fact that the boiling-point of the two liquids is so different—the ether boiling at the temperature of the body, *i.e.*, 96° Fahr., while chloroform boils at 142° Fahr.—must render the mixture on exposure to the atmosphere a very inconstant one, and especially so when warmed by the air from the lungs.

Whoever, then, uses the mixture may depend upon it that the effect he gains is chloroformic; and let him proceed with corresponding caution.

I should not question the statement of any one who would say that he had produced perfect anæsthesia with a drachm and a half of the mixture (*i.e.*, ℥ss of chloroform and ℥j of ether), since Dr. McDowell, Professor of Surgery in Trinity College, says of chloroform, "I have known thirty drops to suffice."

Statistics show that there is one death in 5588 administrations of this combination, making it but little less than half as fatal as chloroform alone.

But I may be asked to express an opinion as to the relative safety of the two anæsthetics.

I was fortunate enough to give chloroform and ether to five persons under similar circumstances, and found by comparison that chloroform—drachm for drachm—was eleven and four-sixths times stronger than ether. In comparing ten cases as nearly alike in age, sex, and strength as I could, I obtained a similar result.† But the relative energy of the two anæsthetics will appear to a greater advantage if we recall the mode of their administration: with chloroform it was sufficient to moisten, drop by drop, a surface about three inches square; while with ether to accomplish the same result a surface three inches wide and sixty long was required.

Herein will be noticed a very surprising difference between the two anæsthetics, and to the reflecting mind this will convey a most important lesson, and suggest a possible explanation for the sad contrast in their career. In less than a quarter of a century, hundreds of unfortunate human beings have met, through the agency of the one, a sudden and appalling death; while with the use of the other, safety is guaranteed with the most ordinary care.

But I will not do chloroform the injustice to say that the guilt is wholly its own. Four thousand cases,‡ representing every phase of society and embracing every age and condition of body, are proof against such an attack.

At whose door, then, lies the blame? Partly at its own, but mainly, I believe, at the thresholds of its adherents.

I say partly at its own door, for I do not think that any one has ever yet ascertained the lethal energy of chloroform. I am confident that I can influence the *average adult patient with less than a drachm and a half of chloroform.* Of this amount, scarcely a third accomplishes its mission; the other two-thirds are lost. Hence, I say, to the energy of the agent itself may be attributed a part of its fatal consequences. In the *Lancet* is a case where twenty minims are said to have caused death. Once I could not have believed this; but the more I employ chloroform the more I am struck with amazement at its magical powers.

But I have charged the disastrous effects of chloroform mainly to those who administered it. Kidd long ago noticed the significant fact that ninety per cent. of the deaths from chloroform were during trivial operations, and he was not backward in assigning the true reason. He says "that chloroform is given into the hands of *hospital-dressers, students, and female nurses,* and that minor operations are usually undertaken without a due corps of competent assistants."

Very few taking charge of the anæsthetic have the least conception of the responsibility of their post. They seem to feel that they must *sponge, control hemorrhage, hand instruments, and have a general supervision of the case, or they are not really useful.* An interne in one of our hospitals is reported to have left the towel over a patient's face and to have gone into an adjoining room for a needed article; and once while I was operating for a friend he left the towel over the patient's face, and came around the bed to see the progress of the operation.

How often have physicians gone *alone* to their patients, because the patient objected to having a "strange doctor" present, and there for some trifling operation administered chloroform, assisted only by persons ignorant both of the agent and of the approaches of danger! I was never frightened but once in the use of chloroform, and that was in the case of a lad whose knee I wished to examine under its influence. Having induced anæsthesia, I handed the towel to a female attendant who had assisted me before in the same capacity. The lesson was a fearful one; and had my patient died I might have been fortunate enough to find a "fatty heart," but

* Dr. Henry Leffmann.

† Snow says that chloroform is more than ten times stronger than ether by measure.

‡ Record of Prof. Gross. See Principles of Surgery, under Anæsthetics.

never after would my conscience have been free from remorse.

While, then, I would exonerate chloroform from the charges that have been laid upon it, I cannot, with the fearful list of casualties before me, do less than express a deep regret that it is still so extensively used by the profession. That it possesses certain advantages over ether, I am fully aware; but that these advantages are greatly magnified, I do not hesitate to affirm. In my own experience I have not found ether more irritating to the larynx, more tardy in its effects, or more likely to produce delirium, than chloroform. I make these statements after a long and faithful experience of both of these agents, upon myself and upon others, and I feel confident that others will experience like results if they will once *learn how to give ether*.

Study, then, the nature and effects of ether. It presents not a single physiological difficulty that care and experience will not overcome; and, when you have once obtained *the secret*, you will feel amply repaid in the security of your patient, and in the fact that you may give your whole attention to your own procedure, without the furtive, anxious glances that are necessary to the successful employment of chloroform.

CASES OF HYDROCELE COMPLICATED WITH VARICOCELE AND CYSTOCELE, TREATED WITH ERGOTIN AND TINCTURE OF PERCHLORIDE OF IRON.

BY CHARLES D. GREEN, M.D.,
Philadelphia.

CASE I.—Hydrocele of the tunica vaginalis, and encysted hydrocele of the spermatic cord.—S. B., aged 19 years, resident of Philadelphia, came under my care Jan. 2, 1873. He had always enjoyed good health, with the exception of having had for the last nine years a hydrocele of the right tunica vaginalis. Twelve months previous to seeing me, the swelling had attained the size of the doubled fist, and the radical treatment for hydrocele was resorted to with apparent success, injection of iodine being used. Some time after, however, a small swelling appeared in the upper part of the scrotum, and gradually increased in size to that of a turkey's egg, lying in front of the pubes, in the site of the external abdominal ring, passing somewhat into the inguinal canal, and also down towards the testicle, which could be distinctly felt in the lower part of the scrotum. The swelling could be pressed slightly downwards or upwards; there was no indication of hernia, no remarkable impulse on coughing; no alteration in size or position as the patient altered his attitude, and when a candle was used the tumor was seen to be transparent. He had been refused admission to the naval service in consequence of his affection, and was anxious for a cure.

Jan. 4.—With a small trocar and canula about six ounces of clear-colored fluid were drawn off, and one grain of ergotin, with twenty drops of distilled water, injected into the sac and allowed to remain. The canula was then removed, and cold applications ordered in case of too much heat and tenderness.

5th.—Patient had not suffered much, though there was considerable swelling of the whole scrotum.

9th.—Since the 6th, the swelling had diminished almost entirely. In the site of the original disease there was a hard, firm mass, about the size of an almond.

10th.—Dismissed, cured. Saw him in June last; parts were normal.

Case II.—March 13, 1874. *Hydrocele complicated with epiplocele*, etc.

A ship-carpenter, temperate, and of strong fibre. Twelve months previous to consulting me, an inguinal hernia appeared on the right side, which, after application of leeches while the patient was in the horizontal position, returned spontaneously. Subsequently it often came down, but it was speedily reduced by change of position. Six months before my seeing him, on one of these occasions, it could not be returned, although leeches and cold and warm applications were had recourse to, and the swelling remained in the canal and superior part of the scrotum. At the bottom of the scrotum was another swelling, about half the size of the first, soft, elastic, and transparent, seemingly quite distinct from the one above, and presenting all the characteristics of hydrocele. The upper swelling was soft and doughy in feel, and all attempts at pushing the protruding parts into the abdomen were abortive. The patient was desirous of proceeding on a long voyage, and anxious for something to be done before he left, as he felt some inconvenience from the size of the scrotum, produced by the two swellings.

On puncturing the lower part of the scrotum, about twelve ounces of clear-colored fluid was drawn off, but no change was perceptible in the upper part of the swelling. Three-fourths of a grain of ergotin and twenty drops of distilled water were injected and allowed to remain. Immediately after, he experienced a smarting pain in the testicle and some uneasiness in the abdomen. Both, however, subsided after a couple of hours.

16th.—He had had no return of the pain, and the swelling was nearly the size it was before the operation, but free from pain or tenderness. Ordered to remain in bed and continue to support the part in a sling.

19th.—Swelling was almost entirely gone; testis could be felt distinctly, of its natural size and in a healthy condition. Dismissed, cured.

Case III.—Hydrocele (of tunica vaginalis) complicated with varicocele.—Jas. H., aged 73, consulted me April 15, 1874 (a lawyer, of temperate habits, rarely ever sick, and active for his age). Four months previous to seeing me, after a trifling strain in lifting books, he first noticed an unusual enlargement of the scrotum, observable during the day, but disappearing in the course of the night. In about a fortnight it became stationary, and remained so up to seeing me, excepting that it was gradually increasing in bulk. The scrotum was about the size of a quart jug, and of a pyriform shape, the skin very tense; the swelling, which was on the left side, extended as high as the external abdominal ring, and had all the ordinary indications of hydrocele of the tunica vaginalis.

April 16.—Scrotum was tapped, the fluid amounting to over two and a half pints, and one grain of ergotin and twenty drops of distilled water injected into the sac and allowed to remain.

18th.—There was considerable effusion since the 16th; scrotum red and tender.

20th.—The later symptoms ameliorated; but the effusion as great as on the 18th.

21st.—Swelling much less, and appeared to depend as much on the enlargement of the veins of the scrotum as on effusion in tunica vaginalis.

22d.—Effusion gone; veins of scrotum enormously enlarged. Injected two drops of tincture of perchloride of iron at three points in the varices, strapped the scrotum, removing the strips the next day; the points of punctured veins were hardened, but showed no active sign of inflammation; no dressing other than supporting the scrotum was used.

24th.—Dismissed.

Saw this patient in August last. No sign of hydrocele or varicocele, the dartos keeping the scrotum well corrugated.

Case IV.—Hydrocele (of the tunica vaginalis) complicated with cystocele, treated with ergotin and perchloride of iron.—John L. J., from Dover, Del. (a tiller and hedger), æt. 62, temperate habits. Five months previous to seeing me, while getting out timber, he felt something suddenly give way in the left groin, not followed by much pain, but by a large swelling of the scrotum.

June 18.—Scrotum tense and painful on pressure, and elastic; testis occupying the anterior middle, the swelling presenting more the appearance of hæmatocele than of hydrocele. Drew off fourteen ounces of chocolate-colored fluid, and injected half a grain of ergotin, with twenty drops of distilled water; allowed to remain. No severe pain; further examination revealed unusual enlargement of the spermatic veins; testis of usual size, and apparently healthy.

19th.—Effusion as great as at the time of tapping yesterday, but no redness or pain except near the external abdominal ring.

21st.—Effusion gone, but some tenderness in the course of the spermatic cord.

23d.—Could distinctly feel and separate the veins from the vas deferens. Having applied a compress over the pubes, injected two drops of the tincture of perchloride of iron in the middle of their length, making three punctures (using the hypodermic syringe). The pain was sharp and smarting, and increased on removing the compress from over the pubes some three hours after; ordered evaporating lotions and support to the scrotum. Operated at 10 A.M. Saw him again at 10 P.M.; was surprised to find the amount of coagulation; one clot large as a hulled walnut, a little tender.

24th.—The clots firmer; no increase in size, but the walls of the spermatic vessels resisted being pressed together, and from this time they underwent rapid diminution, no blood circulating through them.

This patient went home on the 29th of June. I saw him in August. The spermatic vessels had so far disappeared and the remains of them were so cord-like in feel as to make it difficult to distinguish them from the vas deferens.

Remarks.—Out of thirty-five cases thus treated I have not found an undue amount of inflammation, yet the inflammation has always caused such organic changes in the serous lining that adhesion or absorption followed.

In all the cases the effects of the use of ergotin appeared remarkably mild, equally suitable to old age and youth; and the return of the parts to their natural condition seemed more speedy than after the use of iodine, port wine, or even the primary galvanic current, which latter I have used over twenty-two years.

It might be a question whether, in these examples, the cures were in reality complete, as some of the parties were seen so short a time after operation (some living at a distance); but I have no doubt on the subject, as out of many instances where I pursued a similar practice I have never had a patient return with the disease again upon him, and the worst of the above cases I know, from personal observation since, to be cured.

Case I. was somewhat peculiar; I termed it encysted hydrocele of the spermatic cord, under the impression that it was so. If such were the fact, it was, it would be observed, an instance of this form of disease (comparatively a rare one) occurring after the successful performance of the radical operation for effusion into the tunica vaginalis. I thought possibly the collection of fluid had taken place in the upper part of that cavity where adhesions had not been produced; but my experience went against this view, for if adhesions in any case had been caused throughout almost the entire extent of the serous surfaces, the actions in any non-adherent part were so altered that the disease rarely, if ever, recurred. Besides, if this had been the state of affairs, in all probability the recent adhesions would have yielded in such a manner that the swelling would have increased in a downward direction, and occupied far less of the inguinal direction than it did in reality.

Case II. presented a variety, inasmuch as there was an irreducible hernia present as well as a hydrocele. The hernial sac was, in my opinion, filled with omentum, which had become so altered in shape and consistence, and possibly also adherent, as to be irreducible.

I would not have resorted to more than simple tapping, for the radical operation could not be discerned so free from danger as in an ordinary case. The inflammation might extend from one serous membrane to the other, and both being so contiguous the effects might be alarming. The patient, however, was about to proceed to parts of the world where he might not have the advantages of surgical advice; and even in the event of his being seen by some other surgeon, with the hydrocele perhaps much increased, there might be greater difficulties and dangers under such circumstances than were at present apparent. I accordingly used the injection stated, but not without having made sure there was no direct communication between the two sacs. Fortunately, there was no extension of the inflammation, and the case might be considered as simplified in character, there being only one disease instead of two, the remaining one being probably in that condition that it would never alter. In this instance the patient had complained of considerable pain in the abdomen immediately after the operation, which, under the circumstances, caused me more anxiety than common examples. My anxiety, however, was allayed by the fact that some persons feel great pain in the course of the spermatic cord after injection of iodine, etc., extending to the loins and even to the whole abdomen; and doubtless this was the case here, and not from any continuous action on the peritoneal surface.

For the last seven years I have had frequent opportunities of witnessing the effects upon animals of ergotin injected in the tunica vaginalis communis, in producing adhesion or absorption of the lost fluid in the sac. On examining the pig (the animal selected) after operation, in nearly every instance there was firm adhesion, and the sac was so altered anatomically as to defy the distinction from the tunica albuginea: the cases noted extended to fifty-six, including those in which I injected tincture of the perchloride of iron in the cord and spermatic vessels, to produce atrophy of the testes instead of immediate castration. The age of the animal was from

two to eighteen months. The large size of this animal, the active circulation of the blood through its spermatic vessels, and the facility presented by the number so easily obtained, have given me almost unlimited number in one year for experiment, as all the males are castrated as early as possible.

Case III. occurred in a person seventy-three years of age, a period of life at which it is seldom advisable to operate, on account of the danger of shock to the system; the patient should be content with occasional tapping; but here I acted otherwise, being induced to do so chiefly on account of the hale and comparatively robust condition of the parts affected and the mildness of this treatment. The treatment of this and the fourth case resulted in a rapid and, to present appearance, permanent cure.

Case IV., apparently so serious, is now following the laborious business of *tilling*, *ditching*, *hedging*, etc., and declares himself to be *as well as he ever was in years past.*

NOTES OF HOSPITAL PRACTICE.

PENNSYLVANIA HOSPITAL.

SURGICAL CLINIC OF DR. T. G. MORTON, NOVEMBER 22.

Reported by J. J. KIRKBRIDE, M.D.

DR. MORTON presented to the class several specimens, illustrating a gunshot wound involving the knee-joint, the popliteal artery, vein, and nerve, and gave the following history of the case:

My friend R. H. Wevill, M.D., a prominent practitioner, residing in the lower section of the city, aged 45,* received a pistol-wound in the left knee, Nov. 17, 1874; the ball entered on the inner side of the knee, opposite the internal condyle, an inch below margin of the patella, passed through the popliteal space, then upward to the outer side of the thigh, and was found immediately under the skin on the outer part of the biceps muscle, and was extracted by Dr. Nebinger at 6 P.M. the same day. At 6.30 P.M. I saw the patient, and found him suffering intensely from pain in the limb, which was cold below the knee; very great swelling existed in and adjacent to the popliteal region and calf of the leg; there was no pulsation in either the anterior or posterior tibial arteries, and there was total loss of sensation on the outer side of the foot, with a general numbness of the entire leg; there was considerable shock, a quick pulse, a cold skin, and a blanched countenance. He had taken two grains of morphia, and had the knee leeched on his own responsibility. At 10 P.M. I had a consultation with Drs. Nebinger and J. H. Packard; we found marked heat in the leg, but no pulsations could be felt; there was continuous shock; pulse 115; skin cold. A wound of the popliteal artery was diagnosed, with perhaps involvement of the vein and contusion or wound of the nerve.

It was decided to envelop the limb with cotton, to apply hot laudanum fomentations around the knee, to administer milk-punch and liquid nourishment, and to await reaction. We again met in consultation Nov. 18, at 12.30; pulsation was now found in the anterior tibial artery, with undue heat in the limb; marked pulsation also existed in the ham, with distinct aneurismal bruit.

The consultation decided that the ball had opened the artery, forming a false aneurism, and that an exploratory operation should be made; that after opening the popliteal space in a median line, should the artery and vein be found injured with any joint-complication, amputation should be performed, or to be guided by the circumstances.

After etherization, I made an incision in a median line five inches long; on reaching the cellular tissue, an immense mass of clotted blood was found, which extended down to the femur; the finger passed in found loose fragments of bone and evident involvement of the knee-joint; the deep tissues and blood-vessels were so extensively lacerated that it was determined to amputate the thigh, which was done by rectangular flaps; little or no blood was lost. The patient bore the operation well, and the stump was brought together with silver wire, dry dressings were applied, and a firm roller from the groin down.

Examination of the limb.—The popliteal region was found infiltrated with blood, which extended down below the soleus and gastrocnemius muscles in large quantities; small spiculæ of bone were found among the clots. The external popliteal nerve was severely bruised, having been brushed by the ball. The popliteal artery was opened, presenting a roughened oval wound, rather more than an eighth of an inch in diameter; the vein was wounded in two places, the ball making a wound of entrance and a wound of exit; both these openings were double the size of the wound in the artery. A small fragment of bone was found driven into the substance of the artery near the opening. The ball passed directly through the inner tuberosity of the inner condyle of the femur, from below upwards, producing a comminuted fracture, the wound of entrance being small, while the wound of exit showed much splintering. The interior of the joint presented the synovial membrane intensely congested, and there was marked increase of the synovial fluid.

Since the operation, the patient has been doing well.

* Dr. Morton, on account of his legal connection with the case, has very properly refrained from giving any statement in regard to the circumstances of the shooting of Dr. Wevill. There are several versions of the affair afloat, and until the legal investigation is made it is impossible to arrive at the exact truth. The following narration is, however, probably substantially correct. Dr. Wevill received one or two notes from the husband of a lady whom he had previously attended, asking him to visit her professionally. He did so, and whilst at the house the lady asked him up-stairs to see some new furniture, or upon some other pretext. Directly after this the husband came into the room with a writing in one hand and a cocked pistol in the other, and demanded that the doctor should sign the paper, which was an acknowledgment of his having had criminal connection with his patient, threatening to shoot him if he refused. The doctor did refuse. An altercation ending in a struggle ensued, and the pistol was discharged. No one, so far as we know, believes that the charges against the doctor had any foundation in fact. His friends seem equally divided between the belief that the shooting was the result of an insane jealousy, and that it grew out of an atrocious attempt at blackmailing.

HYPODERMIC USE OF QUININE (*New York Medical Journal*, November, 1874).—Dr. Alfred A. Woodhull reports three cases in which he has used, hypodermically, a solution of quinine of about six grains to the drachm.

The first was a case of quartan intermittent; the results were about the same as follow the oral administration. The only immediate local effect at each puncture was the death of the skin and its discoloration over a spot about a fourth of an inch in diameter. The second was a quotidian intermittent, which was cured. But in this case every injection was followed by the local death and sloughing of the tissue infiltrated, the sloughs being circular, and varying from three-fourths of an inch to an inch and a quarter in diameter. The patient was incapacitated from work for five weeks.

The third case was one of typhoid fever, in the early stages of which the injections were given. Fourteen of these were followed by local sloughs as in the preceding case, and were painful and annoying for weeks.

PHILADELPHIA
MEDICAL TIMES.
A WEEKLY JOURNAL OF
MEDICAL AND SURGICAL SCIENCE.

The Philadelphia Medical Times is an independent journal, devoted to no ends or interests whatever but those common to all who cultivate the science of medicine. Its columns are open to all those who wish to express their views on any subject coming within its legitimate sphere.

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EDITORIAL.

HOSPITALISM.

A VERY noticeable feature of the papers upon hospitalism read at the late meeting of the American Health Association, and of the subsequent debate, was the singular unanimity in regard to the object to be attained and the singular diversity in the methods of reaching it. It is a universal therapeutic rule that when a large number of remedies are strenuously recommended by different authorities in any disease, no one of them is in any sense a specific, and most of them are about of equal value. We cannot help thinking that this rule holds good in the present instance, and that probably there is not much difference in the working value of the different plans; that the results depend more, far more, upon the *management* of the hospital than upon its mode of construction; that a good medical superintendent is of more consequence than any especially recommended system of ventilation; that in scrupulous cleanliness and an avoidance of overcrowding, and in the watchful care of one who is thoroughly trained for his work and has the brains to use such training, are to be found the requisites for success in hospital management. Beyond this, the problem seems to us largely as to the individual needs of the proposed hospital, and the comparative expense of the various plans.

As every case of sickness demands an individual study, so does every hospital problem. The amount of ground at the disposal of the institution, the nature and situation of that ground, whether the institution is or is not to be used for clinical purposes, etc., may seem unimportant when compared with the question, What is best for the patients? Yet these secondary matters are really of very great importance; and, when it is evident that many plans are of about equal utility so far as concerns the welfare of the sufferers, such secondary considerations must be decisive.

In regard to the question of comparative expense, we have yet to see a fair estimate of the various plans made or even attempted. The first cost of a hospital is but a small part of the outlay. What folly, then, to make this the entire basis of comparison! The plan of a hospital undoubtedly does influence very greatly its running expenses, and a fair comparison between two rival plans must take this into consideration.

The great differences of opinion ought to teach medical writers humility, and all should remember the possible influence their words may have upon the benevolent. To denounce in unmeasured terms our present hospital systems is certainly unnecessary and injudicious; and, indeed, in the light of the results obtained by Mr. Callender in England, by Dr. Goodell in this city, and in the Pennsylvania Hospital, such language seems to us to partake of the nature of folly.

The Pennsylvania Hospital is old-fashioned in the extremest sense of the term. It has had its wards full of the worst surgical cases during many decades; and yet we doubt whether any private practice can show a better result than is there obtained. Until some one points out a fallacy in the statistics offered by Dr. William Hunt in his recent letter to the *Times*, we hope to hear fewer tirades against large and permanent hospitals. If surgeons in whose wards the evil results are obtained would once get the idea that perhaps their personal management of their cases and of their wards ought to bear the blame which they are so eager to lay upon "faulty construction," we should probably have fewer deaths inside and less complaints outside of hospitals.

In all astronomical problems the "personal equation" of the operator is always ascertained beforehand and taken into account. How much more important is the "personal equation" in surgical practice! Yet what medical statistics take notice of it? Comparative statistics, unless when the differences in them are very great, are a fraud and a delusion; and we doubt if any one will ever obtain a result much better than that of the surgeons of our old medical landmark.

LEADING ARTICLES.

THE TRANSMISSIBILITY OF TUBERCULOSIS.

I.

THERE is no question in modern pathology, human and comparative, fraught with greater interest and importance than this of the probable, nay, certain, transmission of tuberculosis from mankind to animals, and, possibly, from these to the human species. The solution of this problem is not only of urgent importance to sanitary science and the preventive medicine of the future, but so seriously concerns us individually of the present day as to demand our closest attention.

It becomes our imperative duty, then, to ascertain whether we are actually engrafting upon our children the fatal malady of tuberculosis by inadvertently administering to them milk derived from phthisical cows, as the experiments made by Klebs, Böllinger, and others lead us reluctantly to believe. It appears, also, further that we are all exposed to nearly the same danger of infection in eating the flesh of cattle, when, as not unfrequently is the case, it is loaded with pernicious tubercular matter.

The revival, in certain quarters, of the belief that tuberculosis is contagious, even in the human species, as Morgagni asserted more than a century ago, suggests the reflection whether we, as physicians and teachers, lay sufficient stress upon the great danger of allowing healthy people, especially young children, to associate with those who are consumptive.

We have yet, it is true, a great deal to learn with regard to the pathology and communicability of this disease; but there can be no hesitation in recognizing the importance of the results already achieved by careful experimentation and observation. Startling as the above assertions may appear, that milk and flesh, such important articles of our food, as well as mere cohabitation, may prove media for the conveyance of tuberculosis, yet nevertheless, in the presence of so many important facts, we must yield them our credence. In short, we fully concur with Chauveau, who says, "It now appears proved that the identity of tuberculosis with the other virulent diseases is so complete and so absolute that we must either recognize its virulency, or deny the existence of virulence altogether. There is no middle place in this dilemma."

And now for our facts. But first let us see how the authorities agree as to the histological identity of human tubercle with that occurring in the lower animals. A remarkable unanimity of opinion exists upon this point at the present time. Professor Reynal,* of the Alfort Veterinary School, stands almost alone in maintaining that the bovine and human tuberculosis are essentially different in their histological features. While admitting that the elements of the bovine tubercle are incontestably identical with those of sarcomatous tumors, this writer, nevertheless, sees proper to separate it from the latter on account of the absence of nutritive vessels and the determinate volumes which can never be exceeded by the tubercular new growth. The majority of the authorities, however, appear to take the opposite view; of these, Prof. Schüppel,† who has made an almost exhaustive study of tubercle in general, and particularly that of the bovine species, has unquestionably fully established the absolute identity in structure and development of the cattle-disease with tubercle in man. Accepting, then, the fact as fully established that human and bovine tuberculosis are identical, the numerous and apparently authentic and exact observations—a few of which will be given later on—as to the extension and transmissibility of tuberculosis in the different species become somewhat startling; but the results of experiments undertaken by various authorities with the view of ascertaining whether the disease could be produced by inoculation with or the ingestion of tuberculous matter, are yet more so. As most of these experiments are no doubt already familiar to many of our readers, we shall do no more than summarize the conclusions drawn from them, excepting a few of the more recent ones, to which, perhaps, as yet, less prominence has been given.

So prevalent was the tradition of the inoculability of tuberculosis that even Morgagni abstained from making autopsies upon the phthisical, and Laennec alleged that he had acquired a tubercular ulceration upon his finger in this manner; yet, strange as it may seem, and despite this long-noted tradition, it was reserved for Villemin to establish, in 1865, by actual experimentation, the certain transmissibility of this affection by inoculation. The animals experimented upon were chiefly rodents,—rabbits and guinea-pigs,—and the material, tubercle from the human lung. The number of animals inoculated was large, and tuberculosis, more or less extensively developed, constantly resulted. No matter in what part of the body these inoculations were made, the tuberculization that followed was marked by the same serious characters in all, many of the creatures dying after a variable period, and others, falling into a state of marasmus, were killed. The lesions noted in them were chiefly found in the lungs, which were more or less infiltrated with tubercles; but other organs and textures were not exempted,—the spleen, liver, bronchial and mesenteric glands, etc., being also involved in many cases. Experiments were also made upon ruminants and carnivora (sheep, dog, and cat), but the results with these were generally negative; though a few of them so inoculated were found with the characteristic gray granulations and cheesy deposits in the viscera. Villemin also determined by further experiments that the tuberculous matter of animals, especially that of the bovine species, could give rise to tuberculization. From these experiments he concluded that bovine

* Reynal, "Traité de la Police sanitaire des Animaux domestiques," Paris, 1873, p. 701.

† "Ueber die Identitäte der Tuberculose mit der Perlsucht," Virchow's Archiv, Bd. 56, S. 38, 1872.

phthisis is identical with that of man. The next step was to ascertain whether the tubercular matter thus produced by inoculation was also capable, like that which arises spontaneously, of being transmitted; or, in other words, whether its virulence was retained beyond the primary transmission. The second generation of tubercular matter was found to have retained all its morbid activity, causing intense tuberculization in an equally short period. Villemin concluded from his experiments that phthisis in mankind was the result of a specific or virulent agent introduced into the organism.

Since the first report made by Villemin, numerous other experimenters have tested the validity of his statements, so that the successful inoculation of the lower animals with tubercular matter has ceased to be a novelty. Among these experimenters we find the names of Colin, Vulpian, Lebert, Cohnheim, Frankel,* Papillon, Nicol, Roustan, Laveran, Empix, Waldenberg, Wyss, Wilson Fox,† and Burdon-Sanderson; of these, the famous papers of the latter,‡ so extensively quoted by English writers, are probably the best known to American readers. All these experimenters succeeded in developing in rabbits and guinea-pigs a disease analogous to tuberculosis by inoculating them with tubercular products (gray granulations, caseous matter, and the sputa of phthisical subjects). It is evident "that the certain result of inoculation of tuberculous matter, of whatever kind, is to produce granulations in the lungs and other internal organs; that these granulations are anatomically identical with gray tubercle, that they become yellow or opaque, and eventually break down into a soft cheesy material. It is further admitted on all hands that the process is, so to speak, a self-destructive one; that in the act of inoculation a new element is introduced into the inoculated animal at the point of insertion, from which it is disseminated to all parts of the body through the lymphatics and arterial circulation."§ This latter statement is not invalidated in the least by the fact that the same effect may be brought about by the use of non-tubercular matter or the application of irritating setons, or other substances acting chiefly as mechanical irritants.

The results of these experiments seemed, it is true, to lead to views antagonistic to the idea of the specific nature and virulence of artificial tuberculosis; but they have been explained by stating that caseous deposits were first formed at the point of inoculation, or where the seton had been introduced, next in the neighboring lymphatic glands, and that it was the caseous matter thus formed which gave rise to the production of tubercles throughout the economy. This explanation is, moreover, entirely conformable to Niemeyer's theory of the development of tuberculosis in man,—a caseous deposit in any portion of the body (lungs, glands, articulations, bones, etc.) being, above all other things, according to this authority, a predisposing cause to tuberculosis; and if the lungs are so frequently tuberculous, this is because in them oftener than anywhere else this caseous matter is formed. He further pointed out that the infection of the blood does not always arise through a cheesy deposit, as alleged by Bühl, since cases of acute tuberculosis occur in man in which no trace of such a deposit exists.¶ He, with others, believed in a gradual generalization of tubercle through the medium of the lymphatics, while Lebert** thought that capillary embolism was always an important factor in the artificially-engendered tubercle. Bühl's theory seems, however, to have been generally accepted in so far as experimental tuberculosis in animals is concerned.

Villemin†† performed a further series of experiments to meet the objections brought against his views, and presents, in an address delivered before the Imperial Academy of Medicine in 1868, some very telling and convincing arguments with regard to the production of tuberculosis by inoculation. In these experiments he sought to realize, in principle, the conditions of a real inoculation,—to wit, a very small wound and an inconsiderable quantity of inoculable matter. Although these conditions were never departed from, tuberculizations extremely variable in their intensity and generalization were obtained; every degree being noted, from a few isolated granulations to those startling generalizations in which every viscus was crammed with the tubercular product, a manifest proof that *the intensity of tuberculization is completely independent of the quantity of matter inoculated.* Observing that the alterations in the lymphatic vessels and glands were far from constant, they often remaining unaltered, and the local tuberculization at the point inoculated was often quite rudimentary, while the viscera of the animal were richly tuberculosed, therefore, says Villemin, *the number and extent of the internal lesions have no relation to the local lesions at the seat of puncture.*

The transmission of tubercle has been likened to a process of grafting; "but how can such a process explain the myriads of tubercular granules which stud the parenchymatous and serous organs?" asks Villemin. "Grafted tissues continue to live and develop at the places where they are deposited, but they are not reproduced elsewhere in the organism. This theory, at the most, could only explain the development of tubercle at the seat of puncture; and even then the inoculated tuberculous matter, to do this, must be endowed with a very active vitality. But the softened matter in the centre of a tubercle does not even contain any distinct elements; we inoculate only with a *detritus.* How can grafting explain the successful inoculation with sputa,—*sputa which has dried for twenty days,* as has occurred in our experiments? Does not all this prove that the inoculated matter acts by virtue of a principle inde-

* Virchow's Archiv, Bd. xlix. S. 216, 1868.

† "On the Artificial Production of Tubercle in the Lower Animals," London, 1868.

‡ Sanderson on the Inoculability and Development of Tubercle, 10th and 11th Reports Med. Officer of the Privy Council, 1867-68.

§ Sanderson, loc. cit.

¶ Niemeyer, "Pathologie und Therapie," Bd. i. S. 280, 7 Aufl.

** Virchow's Archiv, Bd. xli., xliii.

†† "Études sur la Tuberculose," Paris, 1868.

pendent of the histological elements entering into its composition ?"

The *quasi*-tubercles induced in man and animals by various parasites, the microscopic appearances of which so closely resemble those of tuberculosis, are discussed by Villemin, as well as the results of injections of mercury, tallow, dust, and irritant substances of various kinds, even pus, into the bronchia and veins, and the great difference between them and true tuberculosis pointed out. To confound the former with the latter would be as great an error as to identify the pustules produced by tartarized antimony with those of variola.

Demet, Paraskev, and Zallonis, of Syra, Greece, not content with their success in transmitting this disease to rabbits by inoculation with the sputa and blood from a man affected with phthisis, performed the unprecedented and not-to-be-repeated experiment of inoculating a human patient whose history afforded no suspicion of a tuberculous taint, and whose lungs were perfectly healthy, but who was suffering from gangrene of the big toe of the left foot, due to femoral embolism. Amputation of the limb would not be submitted to, and, as a fatal termination was inevitable, a quantity of sputa from a man who had abscesses in his lungs was inoculated in the upper part of the left thigh. Three weeks afterwards, auscultation revealed very slightly increased respiratory murmur at the summit of the right lung, and somewhat prolonged expiration in the subclavicular region. Thirty-eight days after the inoculation, the man died from gangrene; and, on examination of the body, it was found that the upper right lobe had seventeen tubercles in the first stage of development, of a gray color, and very hard. Two granulations each were found in the left apex and upon the surface of the liver. From this unusual experiment, as states Fleming,* from whose paper we have freely abstracted throughout, "it is evident that tubercle is inoculable in man himself, for it is scarcely possible that this patient, who was fifty-five years of age, could have had his organs infiltrated with as many as twenty tubercles in the first stage of development only. Had they been naturally developed, they must have multiplied and passed through their regular evolution, especially in an individual predisposed to phthisis. Their limited number, stage, and size, afford a direct relation to the brief interval separating inoculation from the decease of the patient."

R. M. B.

(To be continued.)

CORRESPONDENCE.

IS FRACTURE OF THE PATELLA AMENABLE TO TREATMENT?

To the Editor of the Philadelphia Medical Times:

DEAR SIR,—I notice in the last issue of the *Medical and Surgical Reporter* an abstract in reference to the treatment of fracture of the patella at the Hôpital Lariboisière, Paris, in which the practice of that institution is stated to be that "It does away with all apparatus, much to the comfort of the patient. The plan is simply to place the affected limb on an inclined plane, and to keep it steady by means of a few turns of a roller, without striving to bring the fragments together; a measure perfectly useless, as, whatever is done, the surgeon can rarely expect anything more than a fibrous union, which, after all, does not interfere with locomotion. What this plan studies to avoid is stiffness of the knee-joint, which so frequently follows prolonged immobility of the limb."

I am particularly impressed with the article, perhaps on account of the fact that some of my own remarks on fracture of the patella at a clinic of the Pennsylvania Hospital, intelligently reported by Dr. J. B. Roberts, are published in the same number of that journal, and because my practice and teaching are so diametrically opposite to those of the Hôpital Lariboisière. The article is, in my opinion, calculated to do harm by its teaching of such practice, endorsed by the influential authority of a prominent European hospital.

I have seen three instances in which fracture of the patella was left without further treatment than a state of rest to the limb; no attempt being made to secure mechanically the coaptation of the fragments. In one case the fracture was associated with other injuries, almost necessarily of a fatal character, but the patient survived for nearly three months. In another instance the fracture was complicated with severe injuries of the limb, which were attended with such great swelling that the fracture was not recognized until after the lapse of more than two months. In the third case the fracture was treated as a simple sprain, with rest alone, by a "homœopathic surgeon," and the patient came ultimately under my care on account of permanent disability of the limb, due to a separation of the fragments of not less than five inches.

In each of these cases the separation of the fragments remained far greater than I have ever seen it as the result of ordinary treatment in which approximation is effected as thoroughly as is usually practicable. These opportunities of observing the natural history of fracture of the patella do not incline me to favor such masterly inactivity in its treatment as is practised in the French hospital alluded to.

As to stiffness of the knee-joint following the mechanical treatment of fracture of the patella, I may merely remark that the resulting stiffness is due to the synovitis and other inflammation of the surroundings of the fracture, rather than to "prolonged immobility," which is not during a longer period nor more thorough than is necessitated in ordinary cases of fracture of the femur or of the tibia. In regard to "a fibrous union not interfering with locomotion," I know from practical observation that a *long* fibrous union, as is likely to result from such want of treatment as is the custom at Lariboisière, does interfere with normal locomotion.

While impressing the facts that much of the separation of the fragments in fracture of the patella is due to the

* George Fleming, "The Transmissibility of Tuberculosis," British and Foreign Medico-Chirurgical Review, No. cviii., October, 1874.

swelling of arthritis and bursal distention, and that coaptation cannot be effected until these conditions are subdued, I always teach methods of mechanically approximating the fragments as soon as may be practicable. With efficient mechanical treatment I have seen a number of cases in which, if true synostosis was not effected, the fibrous union was so short that no separation was appreciable.

After some experience with various mechanical contrivances for the treatment of fracture of the patella, I am convinced that the best results, with the most comfort to the patient, can be produced by the simple use of long strips of adhesive plaster and roller-bandages, and with either the limb elevated or the trunk kept raised, so as to flex that portion of the quadriceps muscle which has a pelvic origin. The strip holding down the upper fragment should be long enough to reach spirally down the leg, continuing even to the sole of the foot. In order to render the dressing very secure, several strips of adhesive plaster may be superimposed, so as to give strength and solidity. Strips passing in a contrary direction readily maintain the lower fragment in position, as there is no muscular influence tending to its displacement; and a roller-bandage, enveloping the entire limb, completes the dressing.

During some recent observation in European hospitals, including the Hôpital Lariboisière, I found no practice in the general treatment of fractures, to which we are in this country not accustomed, that is worthy of our imitation. On the contrary, the treatment of fractures in most European hospitals is often careless, unmechanical, and unphysiological. The most favorable exceptions to this condemnation I saw in one or two hospitals in London. From the general practice of the French surgeons in the treatment of fractures we have surely nothing to imitate. I saw, however, in some European hospitals a fair and creditable attempt at the introduction of what they term the "American methods" of fracture-treatment, particularly in the treatment of fracture of the femur.

I trust that the expectant plan of treating fracture of the patella will not be copied in this country; but if there be those whose fondness for and reliance on French authority must incline them to imitate, I would suggest to them the probability of involvement in litigation for malpractice, in suits in which their surgical brethren might not be able to endorse their practice.

R. J. Levis, M.D.

PROCEEDINGS OF SOCIETIES.

PHILADELPHIA COUNTY MEDICAL SOCIETY.

A CONVERSATIONAL meeting was held October 14, 1874, at 8 o'clock P.M.

President, Dr. Washington L. Atlee, in the chair.

Dr. O'Hara asked if any gentleman present could give him any instruction in the method of strapping the chest in incipient phthisis. Surgeon McCrea, of the Belfast Dispensary, in the *Lancet* for July 18, 1874, recommends a process of strapping with emplastrum roborans. He states that there is a marked diminution of cough, cessation of pain, relief of dyspnœa, and a reduction of temperature by the repose thus given to the chest-walls. The patient at once expresses a feeling of great personal comfort. By repeated applications, once a fortnight, he would hope for brilliant results. When it became loose, the recurrence of the cough admonished the patient and his friends of the necessity of its reapplication. He has seen no account of its trial on this side of the water.

Dr. Welch stated that this subject would probably be brought before the Society during the winter in the form of a paper.

Dr. H. C. Wood called attention to a paper in the current number of the *British and Foreign Medico-Chirurgical Review*, which seemed to him to prove the contagiousness of phthisis, and remarked especially upon the fact, seemingly proven, that a stall will become affected so that healthy cattle placed therein will contract the disease. The bearing of this upon consumption-hospitals is very obvious. The moral and hygienic objections to such institutions are overwhelming, and should cause them to be entirely abandoned.

Dr. Lee said that observations which had been made in the early history of the efforts to treat chronic affections by means of remedial exercises and localized movements had an interest in this connection from a negative point of view. As is well known, physical exercise is one of the most powerful agents for improving nutrition. We find it so in health, and it is not less so in ill health. Now, phthisis is, to a great extent, a disease of impaired nutrition. Hence it was concluded by enthusiastic investigators of this new method of treatment that movement would be just the thing for it. They began vigorously with such exercises as would tend to develop the chest and call into play the muscles of respiration. But they soon found themselves obliged to desist. Their patients grew manifestly and rapidly worse instead of better. They then adopted the alternative of confining their efforts to the muscles of the lower extremities; and by exercising them cautiously so as not to quicken the respiration, they found that they were able to avoid any such disastrous results as were at first observed, and in some instances to produce decided benefit. If, then, increased action of the thoracic muscles is injurious in this disease, it is a fair inference that enforced rest of these muscles, and hence, to some extent, of the subjacent lung-tissue, may be beneficial.

Dr. Atkinson thought this idea militated against the plan of "roughing it," so often suggested and urged upon consumptives. He was sure he had seen marked benefit from this plan in repeated instances, and mentioned a case which he had encountered in California, where horseback-riding the whole distance from New York City had acted in a highly beneficial manner.

Dr. Hamilton said the proposed new method for the cure or retarding the progress of phthisis was, as intimated by Dr. Atkinson, in singular contrast with the practice of physicians now or in earlier times. The late Dr. Joseph Parrish used to narrate in his lectures the history of a number of most striking cases illustrative of the great advantages of much out-door exercise in this disease. So, also, Dr. Benjamin H. Coates, when physician to the Pennsylvania Hospital, made it a point to recommend such of the consumptives as were quite able to take exercise to leave the wards of the hospital, assured that, in so doing, they would protract the issue of the malady. The propriety of this practice is confirmed by the experience of practitioners generally.

Dr. LEE said, in reply, that probably every member present could recall precisely similar cases to those which had been detailed, and that no one would be readier than himself to prescribe just this remedy in the earlier stages of phthisis. But "roughing it," in such cases, meant simply getting an abundance of fresh air; and fresh air, as we all agree, is the panacea in such affections. He believed, however, that just in proportion as our patients, while availing themselves of this invaluable remedy, avoid violent exercise of the muscles of the upper extremities, will they profit by it. In regard to the use of these muscles in horseback-exercise, which has often been found beneficial in phthisis, he would say that, in riding, the muscles which are brought strongly into play are the erector muscles of the spine and lateral muscles of the abdomen, which assist in preserving the equilibrium of the trunk, and the abductors of the thigh, with which the saddle is grasped. But one may ride all day without touching the reins or bringing the arms into powerful action.

Dr. O. H. ALLIS read a paper on Anæsthetics. (See p. 145.)

Dr. H. C. WOOD stated that he had been exceedingly interested in the very practical paper of Dr. Allis, but that he thought the difference of energy in the two agents was not the sole cause of the different mortalities following their use. The English Chloroform Committee years ago established the fact that in the animal, during chloroformization, the blood-force steadily falls from the very commencement, whilst during the anæsthesia of ether the arterial pressure rises and is maintained for many hours, only falling when the respiration is sensibly interfered with.

The same facts the doctor believed to be true in regard to man. When chloroform is inhaled, the pulse can often be felt to grow weaker, whilst after a few drafts at the ether-sponge the wave becomes fuller and stronger. For these reasons, whilst ether may be safely administered to a patient when standing, chloroform should never be given except to a person lying down. Ether stimulates the heart; chloroform paralyzes it. Ether kills through the respiration, by asphyxia; chloroform very often destroys through the circulation, by syncope. Upon these facts, rather than upon the comparative energy of the two agents, Dr. Wood conceives, rests the difference in the danger that attends their use.

Dr. Allis, being asked by Dr. Wood for the cause of death from chloroform, said that he had no theory to offer for its fatal action, other than the theories already familiar to the profession; that he had, however, watched and carefully studied the effects of the two anæsthetics upon himself, and a similar succession of phenomena was produced in every instance; that he was quite certain in his own mind that to a *greater anæsthetic energy*, rather than to any difference in kind, might be attributed the greater frequency of death.

Dr. W. GOODELL said that the paper of the evening was of great practical value, and that he, for one, felt himself under deep obligations to Dr. Allis for bringing up the subject of anæsthetics before the Society. He himself generally preferred ether, as being a more trustworthy agent, but he was now sensible of having made the very mistakes in its administration which had been so clearly pointed out in the paper. In future he should strive to avoid them by carrying out the mode of administration so ably advocated by Dr. Allis. In regard to the depressant effect of chloroform on the circulation, he was entirely in accord with Dr. Wood. Apart from crucial experiments, the method of resuscitating cases of threatened death—viz., by lowering the head and raising the body—so vividly described by Dr. Sims before the British Medical Association, goes to sustain this view. So also does an able paper of M. Campbell, of Paris, in which he contends that the immunity with which parturient women inhale chloroform is owing to an antagonism between the effects of labor and the effects of this anæsthetic. Chloroform induces anæmia of the brain, but the throes of labor congest the brain, and, therefore, act as an antidote.

Dr. ATLEE desired to make some remarks on the position assumed by the lecturer with regard to the use of a combination of chloroform and sulphuric ether. For over twenty years he has been in the habit of administering it, and has never seen any bad results arising from its employment. He uses it in the proportion of two parts of ether to one of chloroform; and introduced this formula to the profession many years ago. It is not a mere mixture. A chemical combination occurs between the two fluids, and heat results in consequence of a condensation in the union of the two fluids, the same as when sulphuric acid and water are united. If it were a mixture, one would separate from the other; but the combination is permanent, as he has kept it unaltered for months. Dr. Atlee believes that the combined liquids vaporize simultaneously and in combination, and not independently and separately as is supposed. Although the boiling-point of ether is about half that of chloroform, and about one-third below that of water, yet the specific gravity of its vapor is about five times greater than that of water.

Dr. Atlee has frequently kept a patient under the influence of his anæsthetic an hour with one and a half ounces of the liquid. His method is to use a starched napkin, eighteen inches by twenty-four, folded lengthwise into three folds, embracing a *Ledger* newspaper, and then rolled into a cone and securely pinned. A few drops only of the liquid are dropped on the inner side of the cone, and the latter placed over the mouth and nose. No sponge is placed in the cone. Solid food is not allowed for twelve or sixteen hours before, and nothing for three hours previous to the administration of the anæsthetic. The patient, in a recumbent posture, is brought slowly under its influence, and the cone is removed on the least indication of heavy breathing. As he has used this combination between one and two thousand times without unpleasant symptoms, it is a matter of demonstration. He should be very sorry to see the use of chloroform forbidden by law. With regard to ether, Dr. Atlee stated that the lecturer had the day before yesterday administered it, by means of his apparatus, to a patient of his, on whom he had performed ovariotomy. He had never seen ether employed with greater pleasure and with better results. There was one effect, however, which, if constant, would render it inadmissible in ovariotomy. The muscles of the abdomen were not sufficiently controlled, and the intestines were forced out, so as greatly to impede the operation. The rigid muscles, after the removal of the tumor, had to be drawn aside by the hands of an assistant before the intestines could be replaced. The operation lasted forty-five minutes, and six ounces of ether were used. Dr. Atlee had used the bichloride of methylene on the recommendation of F. Spencer Wells, but found no advantages possessed by it over ether or chloroform.

Dr. Allis, in reply to Dr. Atlee, said that no one could witness the care with which Dr. Atlee prepared a patient for an operation, and not feel sure that any anæsthetic he would use would prove safe. He said that he was aware that the mixture of the two agents would remain unchanged while in a fluid condition; but the point of still greater moment was their behavior in the gaseous state. The fact that 96° Fahr. is the boiling-point of ether, while 142° Fahr. is the boiling-point of chloroform, must render the vapor given off from the mixture at an ordinary temperature an exceedingly changeable

one; and upon this condition alone he based his reasons for saying that the mixture was a dangerous one.

Dr. LEE said that he had used Dr. Atlee's mixture for many years without knowing to whom he was indebted for it. He had noticed this fact in regard to its preparation, which might have some bearing on the question of its being a true chemical combination: When the two fluids were poured together by the apothecary in a clean vial, the mixture very often presented a milky appearance, which appeared to him to indicate a failure to unite. He was then in the habit of requesting the apothecary to add a small quantity of pure alcohol to the mixture, say in the proportion of a drachm to three ounces, when the milkiness at once disappeared. In regard to Dr. Atlee's criticism on the lecturer's condemnation of this mixture, he had understood the lecturer to animadvert especially upon its reckless use by dentists while their patients were in the sitting posture. In this view of the question he thought too strong language could not be used, and he was sure that Dr. Atlee would be the last to wish to give the weight of his name to the impression that this combination was free from danger when used under such circumstances.

In reply, Dr. Atlee said that if pure concentrated or washed sulphuric ether is used, there will be no milkiness or cloudiness on mixing it with chloroform. Whenever the minute globules of chloroform can be seen floating through the ether on shaking them together, there is some fault with the ether, and a mixture, instead of chemical union, is the result.

Dr. O'HARA, in connection with the remarks of Dr. Atlee, who said he would not forego the use of chloric ether and chloroform unless compelled to do so, remarked that the moral responsibility of using chloroform and ether was very great. He thought the legal responsibility had already been fastened on us in view of the recent verdict of two coroners' juries in Boston, who seemed to be backed by the medical profession there; that medical men ought to be held to a criminal responsibility who used chloroform, which, in a certain number of cases, was death; whereas ether was considered to be harmless. He would also suggest, in reference to the danger in the use of chloroform, that some might use it with immunity. He had seen one person, who had been drinking freely, inhale and drink half a pound without inducing sleep. He must have been saturated with chloroform, as it was present in his vomit during the whole of the next day.

Dr. HAY is familiar with Dr. Atlee's mixture. He has used it for a long time, and has been present at many operations performed by Dr. Atlee for ovariotomy, when this mixture was given. Its use was followed by uniformly good results; it did its work well, and the patients recovered from its effects without an unfavorable symptom occurring.

In a recent case, in administering the mixture to a boy not quite six years of age, for the removal of an orbital tumor, it induced collapse three times, and he was obliged to discontinue its use and finish the operation with the patient under ether. In this case (the operation having been performed in a country town) there may possibly have been some fault or error on the part of the assistant in administering the mixture; but, at all events, the result was as stated. The operation was a painful one, the tumor being a large one, involving the removal of the eyeball and an extensive surface of periosteum, to which the tumor was attached. The time consumed in operating and resuscitating the patient was nearly two hours. The little patient made a rapid recovery, and is now well.

Dr. Allis being appealed to by Dr. O'Hara to explain why chloroform taken into the stomach was not so likely to prove fatal, said, "In the lungs we have fourteen hundred square feet of pulmonary mucous surface, and over this entire surface the blood is spread out to favor rapid oxygenation. To this extensive surface the vapor of chloroform comes; while in the stomach, the chloroform in a fluid and gaseous state comes in contact with a comparatively small area."

Dr. KEYSER remarked that in 1865 he had the pleasure of being present at one of Dr. Atlee's operations, and then learned the doctor's mixture of chloroform and ether, which he (Keyser) continued to use for about four years, when, from having nearly lost three cases by it,—one a boy of ten years, on whom the operation for strabismus was made, another a boy of thirteen years, on whom an iridectomy was performed, and the other a man of thirty-three years, where the eyeball was enucleated,—the mixture was given up, and pure ether used since. He also mentioned that when he was assisting in the clinic of Prof. Von Langenbeck, in Berlin, in 1864, a somewhat similar apparatus to Dr. Allis's was used for chloroform. A single thickness of flannel was stretched across a wire frame, and the chloroform dropped on from a dropping-bottle.

Even ether has given him some trouble and worriment. In one of his cases, lately, at the Wills Eye Hospital, a man some thirty years of age, after he was half under the influence, the breathing and pulse suddenly stopped, and it was with difficulty that the patient was resuscitated, after which the least particle of ether would cause a recurrence of the bad symptoms. His heart and lungs were found perfectly sound.

Prof. MCINTYRE, of Lafayette College, being present, was called upon for his opinion as to the chemical union between chloroform and ether. He said he would not be able to decide that question without experiment.

Dr. KEYSER said he had nearly lost three cases by the use of Dr. Atlee's mixture. He had been an assistant to Prof. Langenbeck, where a similar apparatus to the one described had been used.

Dr. DRYSDALE remarked that ether itself was not exempt from danger, as he himself had recently nearly lost a case from its use.

GLEANINGS FROM OUR EXCHANGES.

EXTIRPATION OF THE SCAPULA (*The London Medical Record*, October 21, 1874).—Dr. R. Schneider reports a case in which the left scapula was extirpated on account of a sarcomatous tumor. The first incision was made along the base of the scapula; the second was commenced at the acromion and carried horizontally along the upper margin of the tumor to the commencement of the first incision. A flap was thus formed, the free apex of which corresponded to the upper and inner angle of the scapula. The portion of tumor which projected beyond the upper margin of the bone was then exposed. The muscles attached to the inner and outer margins of the scapula were next divided. As the acromion, the neck of the scapula, and the acromial part of the clavicle had become involved in the tumor, it was found necessary both to open the shoulder-joint and to remove a portion of the clavicle. The deltoid muscle was cut through at its upper part, and the clavicle exposed and divided at an apparently healthy part. The articular capsule was now exposed, and the long tendon of the biceps muscles, and the tendons of the muscles inserted into the great and small tuberosities, were cut through. The articular capsule was then completely separated from the margin of the glenoid cavity. The axillary portion of the tumor was dissected out with great care, in order to avoid wounding any of the large vessels of this region. The tendons of the muscles attached to the coracoid process having next been dissected, the separation of

the tumor from the side of the thorax was easily affected. The supraspinatus, infraspinatus, and subscapularis muscles were wholly removed. During the operation, the subclavian artery was compressed against the first rib. A spray of carbolic-acid solution was played upon the wound during the operation. The vessels were closed by carbolized catgut, and the dressings were strictly according to Lister's plan. The operation lasted for little more than half an hour, and the hemorrhage was very slight; consequently there was no subsequent collapse.

The patient did well after the operation, and six weeks later was regarded as cured,—with a moderate amount of motion remaining in the left arm. Subsequently, however, the disease returned in the left shoulder near the cicatrix, and finally resulted in his death.

This is the nineteenth reported case of excision of the scapula with preservation of the upper limb. It was removed in two cases on account of caries, once on account of some obscure tumor, three times on account of enchondroma, once on account of an osteo-fibroid growth, and in thirteen cases on account of sarcoma or carcinoma. Of the nineteen subjects of these operations, one only died from the immediate effects. Two patients died from pyæmia, and one in consequence of bronchitis.

Post-Partum Hemorrhage—Injection of Per-Chloride of Iron—Transfusion—Recovery (*The Lancet*, October 24, 1874).—Dr. Edward Henderson reports a case of post-partum hemorrhage occurring in a lady who was rendered anæmic by prolonged lactation, and who had previously had two severe floodings.

After the delivery of the child and the detachment of the placenta, the uterus refused to contract in response to friction, continuous pressure, etc., and a large quantity of blood was lost. After some little delay, iced water was freely injected into the cavity of the uterus, into which a piece of ice was also pushed, and the vagina was filled with ice. Partly by this, and partly by well-sustained compression, the bleeding was at last arrested, and the uterus, regaining a portion of its lost force, contracted fairly. Brandy and champagne were from time to time administered to the patient. At the expiration of two hours and a half, however, the bleeding recommenced; oozing went on rapidly; her surface and extremities became cold and blanched, her breathing sighing and interrupted, her pulse feeble and intermitting. Twelve ounces of a solution of per-chloride of iron were at once injected, with the effect of *immediately and finally checking the hemorrhage*.

Soup prepared from raw meat by the addition of hydrochloric acid and water was given, together with stimulants, but the patient's condition grew more and more alarming. Her breathing was labored and irregular; her voice a mere whisper; her pulse 130, very feeble and intermitting. She said she was dying.

A saline solution containing about two drachms of pure alcohol was then injected into a vein by means of an apparatus acting on the gravitation principle. The change in the patient's condition which followed the operation was no less rapid than wonderful, the marked improvement in pulse, breathing, and general appearance making the prognosis much more favorable. Two hours later, however, all the bad symptoms returned, and her state was, if possible, worse than before. Twelve ounces of blood were then taken from her husband, were strained and defibrinated, and were injected through the same orifice by the same apparatus. The result was favorable beyond the most sanguine anticipations. The pulse once more filled and steadied, the breathing became full and regular, and by slow degrees warmth returned to the extremities. This improvement was permanent; the reactionary fever, though marked, was moderate, and the patient completely recovered, with no untoward symptoms.

Rest in the Treatment of Chorea (*The Chicago Journal of Nervous and Mental Disease*).—Dr. Ransom Dexter reports the case of a girl, æt. 13, whose health was impaired by hard study at school, and who finally became choreic. The treatment at first adopted consisted in the administration of extract of valerian and cimicifuga twice a day, the elixir of pepsin, bismuth, and strychnia before each meal, and bromide of potassium and cannabis indica at bedtime. In a few days she grew worse, and some malarial periodicity was noticed. It was then decided to try cautiously small doses of quinine and iron, but when only four grains of the former had been administered the following symptoms ensued: hemiplegia of the left side, dilatation of the pupils of both eyes, some choreic movements during sleep, increasing so much when awake that the patient was unable to sit up, but had to be held down on the bed or lounge. In reviewing the case, which had then become very serious from the general failure of the vital powers, Dr. Dexter arrived at the following conclusions:

1. That all the sensorial ganglia, or the centres for the nerves of the separate senses, were more or less affected; and that the sensori-motor centres were the special seat of the disease.

2. That the pathological condition could not be other than an asthenic irritation of the sensori-motor organs.

3. That these organs must have physiological and therapeutical rest.

Carrying out these ideas, he ordered the patient to be kept in a darkened room; to lie as still as possible upon a lounge; to be waited upon only by her parents, who were to maintain the strictest quietude; and to be positively silent herself. In addition, the valerian, cannabis indica, and bromide were continued, with an occasional cathartic to act as a revulsive. In eight hours a noticeable improvement had ensued, and in twenty-four hours had become very decided. In six days all choreic movements had disappeared.

The Venom of the Scorpion (*The London Medical Record*, October 21, 1874).—M. Jousset de Bellesme records, at some length, researches on the venom of the scorpion, and gives the following description of its situation and the mode of its ejection:

The poison-apparatus of the scorpion is lodged within the sixth and last caudal segment [probably the homologue of the mesial element, or "telson," of the last segment of the tail of a macrourous crustacean, *e.g.*, a lobster, or crawfish.], which is of a rounded flask-like shape, and terminates in a horny blackish dart, recurved and very sharp. Within the ampulla are contained two little glandular sacs, communicating by a common duct, which extends into the dart to terminate near its point by two longitudinal slits, which are somewhat difficult to see. It is through these that the venom is injected, by the aid of the contraction of muscular fibres, which invest the glandular saccules. In its natural attitude the tail of the scorpion describes a curve; but when the animal imagines itself to be completely sheltered against all dangers, it rests on one side or on the other, upon the ground. It is by a sudden extension of the tail that the dart, after being first brandished over the head of the scorpion, is launched against the body of the victim; and, however small be the prey, it is never brought to the mouth until it has been pierced by the sting. This is a measure of precaution; and that it is not an inherited instinct, but is probably acquired by bitter experience, the following instance will show. M. de Bellesme gave one day to a captive scorpion, which he had hitherto been feeding upon flies, a spider of the size of a pea.

The scorpion, having seized its prey with its chelæ, stung it *en règle*, but, probably owing to some flow of blood, the poison took no effect. The spider, *sicut est mos*, feigned death, and as the scorpion, deceived by this *ruse*, was about to transfer its prey to its mouth, the spider suddenly buried its "mandibles" in the base of one of the chelæ of the scorpion. The latter stung its victim anew, and this time with effect, but had scarce time to enjoy its victory before it began to show signs of discomfort, and was afterwards seized with tetanic spasms, from which it did not recover till the following day, when it ate a fly with good appetite. The scorpion always attempts to sting its prey in the thorax, death taking place much more rapidly when this site is luckily hit upon, the rapidity of its access being in the inverse ratio to the size of the victim.

MISCELLANY.

ORIGIN OF THE TRANSFUSION OF BLOOD.—The following paragraph is taken from Villare's "Life of Savonarola," and is perhaps the earliest recorded case of the operation of transfusion:

"The vital powers of Innocent VIII. rapidly gave way; he had for some time fallen into a kind of somnolency, which was sometimes so profound that the whole court believed him to be dead. All means to awaken the exhausted vitality had been resorted to in vain, when a Jew doctor proposed to try to do so by the transfusion, by a new instrument, of the blood of a young person,—an experiment that had hitherto only been made on animals. Accordingly, the blood of the decrepit old pontiff was passed into the veins of a youth, whose blood was transferred into those of the old man. The experiment was tried three times, and at the cost of the lives of three boys, probably from air getting into their veins, but without any effect to save that of the Pope. He expired on the 25th of April, 1492, and without loss of time they set about the election of his successor."

DEATH FROM SMOKING.—The case is reported in France of a young man who perished in attempting to smoke twelve cigars for a wager. At the eighth cigar he began to feel uncomfortable, at the ninth he had chills and dizziness, which became more severe with the tenth. He still refused to stop smoking, but consented to go home with two of his friends. He was then attacked with vomiting and pain in the abdomen, and, in spite of professional assistance, he died in the night. It should be stated that the heart was hypertrophied.—*Boston Medical and Surgical Journal.*

THE CARRIER PIGEON.—The travelling pigeon never feeds. If the distance be long, it flies on without stopping to take nutriment, and at last arrives thin, exhausted, almost dying. If corn be presented to it, it refuses, contenting itself with drinking a little water and then sleeping. Two hours later, it begins to eat with great moderation, and sleeps again immediately afterwards. If its flight has been very prolonged, the pigeon will proceed in this manner for forty-eight hours before recovering its normal mode of feeding.—*L'Union Médicale; Medical Times and Gazette.*

NOTES AND QUERIES.

3729 LOCUST STREET, WEST PHILADELPHIA.

TO THE EDITOR OF THE PHILADELPHIA MEDICAL TIMES:

MR. EDITOR,—It is an old saw, that "one must go away from home to learn the news." I translate from the Portuguese the following article found in the *Carreio Paulistano*, which speaks for itself:

"SERVICE TO HUMANITY.—Two patients, one with stone in the bladder and the other with one in the kidney, who are in a hospital in Philadelphia, are being submitted to treatment by distilled water. Distilled water, according to the general belief of men of science, has a great solvent power.

"Dr. Moore (?), before proceeding to the operation of lithotomy upon the patient with stone in the bladder, ordered about half a pint of cold distilled water to be given to him an hour and a half before meals. The first effects of the experiment promising good results, Dr. Moore applied the same treatment to the other patient.

"Both of them expelled much very fine sand, and it was judged the concretions which affected them were being dissolved, because their pains had very much diminished. If Dr. Moore should obtain the results desired, he will do a great service to humanity. The treatment is simple, but should be methodical and prolonged."

Also this, from the *Gazeta de Camfimas:*

"We have been favored with the thesis which was presented and defended before the Faculty of Medicine of Rio de Janeiro by our fellow-provincial, William Ellis, a Doctor by the University of Pennsylvania in the United States.

"The subject of his essay is entitled 'Emphysema of the Liver,' into a discussion of which we shall not enter now, not only on account of our incompetency, but because this is a subject upon which men of science maintain silence, and about which there is much controversy. Dr. Guilherme Ellis was *approvado plenamente* by this Academy of the Empire, and proved his perfect preparation for the exercise of the noble profession of a physician in Brazil. We hope ardently for his prosperity."

It is worthy of notice that of three European graduates examined at the same time, one was rejected, and two *approvado simplemente*, the inferior grade to that accorded to Ellis; which facts are rather rough for the theories of the editor of the *Times*.

Yours,

DR. W. H. WINSLOW.

It seems a matter of sheer impossibility to make one's self understood. We have no especial theories upon the subject of medical education, but we have asserted and reasserted the following facts emphatically: to these we suppose Dr. Winslow refers in his closing sentence:—that this city offers the greatest advantages to those desirous of studying medicine deeply, but that such study is not necessary to obtain a diploma; that the real student will find here almost everything he wants, excellent teachers, abundance of clinical material, large libraries, a union of facilities very probably greater than exists in any other city in the Union,—but that the diploma is no *proof* that the possessor has availed himself of these facilities; the graduate of one of our schools is often a brilliantly educated man—but perhaps as often he is a blockhead, or at least an ignoramus.

The case of Dr. Ellis is in close concord with all that has been editorially written in the *Times*. We had the pleasure of knowing Dr. Ellis as an earnest, intelligent student, taking advantage of every opportunity, and, after graduating, serving with distinction as interne in one of our hospitals. Of course he passed a brilliant examination when he returned home. Such Philadelphia graduates we would back against the world.

W.

PHILADELPHIA COUNTY MEDICAL SOCIETY.

A CONVERSATIONAL meeting of the Philadelphia County Medical Society will be held December 9.

Dr. H. C. Wood will lecture on the Use of Electricity in Paralysis.

All regular practitioners of medicine in the city are invited.

OFFICIAL LIST

OF CHANGES OF STATIONS AND DUTIES OF OFFICERS OF THE MEDICAL DEPARTMENT U.S. ARMY, FROM NOVEMBER 24 TO NOVEMBER 30, 1874, INCLUSIVE.

MAUS, L. M., ASSISTANT-SURGEON.—Assigned to duty at McPherson Barracks, Atlanta, Ga. S. O. 191, Department of the South, Nov. 25, 1874.